

XVI. *Reply to Mr. Cavendish's Answer.**By Richard Kirwan, Esq. F. R. S.*

Read March 18, 1784.

I MEAN to trouble the Society but with a very few words in reply to Mr. CAVENDISH's answer, as I consider the greater part of mine to him as still unanswered.

In the first place, he says, that in Mr. LASSONE's experiment the effervescence proceeded not from any fixed air in the alkali, but from the further action of the acid on the zinc from which inflammable air was disengaged. But this could not have happened; for, first, the zinc, instead of being further acted on by the acid, was precipitated according to Mr. LASSONE's own account (p. 8.); and, secondly, the acid was only added by degrees, and undoubtedly would unite to the alkali preferably to the zinc; therefore it was from the alkali, and not from the zinc, that the effervescence arose.

2dly, With regard to the calcination of lead; though in England the smoke and flame may come in contact with the metal, yet in Germany red lead is formed without any communication between them, according to Mr. NOSE, who has given an ample account of this manufactory (p. 86.). Is not lime formed in contact with fuel, flame, and smoke? Mr. MACQUER even thinks it probable, that the contact of flame is hurtful to the production of minium (2 Dict. Chy. 639.). Mr. MONNET made minium by melting lead in a cuppel, in
such

such a manner that it was impossible it could come in contact with the least particle of flame or smoke (Mem. Turin. 1769, p. 71.).

Mr. CAVENDISH expresses his surprise at my asserting, that the black powder, which Dr. PRIESTLEY formed out of an amalgam of mercury and lead, was exactly the same as that out of which he had extracted fixed air; but, I think, I have assigned very sufficient reasons for my opinion: how far I was right will best appear by Dr. PRIESTLEY's own letter, in the hands of the Secretary, of which the following is an extract.

“ I certainly imagined the two black powders you write about to be of the same nature, and therefore did not attempt to extract any air from the latter; but immediately on the receipt of your favour of yesterday, I dissolved an ounce of lead in mercury, and expelling it by agitation, put the black powder, which weighed near 12 ounces, into a coated glass retort; then applying heat, I got from it about 20 ounce measures of very pure fixed air, not $\frac{1}{10}$ th of which remained unabsorbed by water.”

Fourthly, it is impossible to attribute the fixed air, produced by the distillation of red precipitate and filings of iron, to the decomposition of the plumbago contained in the iron; for the quantity of fixed air produced in Mr. CAVENDISH's own experiment is more than *twice* the weight of the whole quantity of plumbago contained in the quantity of iron he used, supposing the whole of the plumbago to consist of fixed air, which is not pretended; and more than *eight* times the weight of the quantity of fixed air which plumbago really contains. For Mr. CAVENDISH employed in his experiment 1000 grains of iron and 500 grains of red precipitate, and obtained 7800 grain measures of fixed air, which are equal to 30 cubic inches, and weigh 17 grains. Now 100

grains of bar iron contain, according to Mr. BERGMAN, at most, two-tenths of a grain of plumbago; and consequently 1000 grs. of this iron contain but two grains of plumbago; and plumbago, according to Mr. SCHEELÉ, contains but one-third of its weight of fixed air; so that here, supposing the plumbago to be decomposed, we can have at most but seven-tenths of a grain of fixed air, or little more than one cubic inch. If we suppose the filings to be from steel, 1000 grains of steel containing eight of plumbago, we may have about 2,5 of fixed air, or about 1,5 cubic inch, and this is the strongest supposition, and the most favourable to Mr. CAVENDISH. What shall we then say, if we consider that these filings were mixed with copper or brass which contain no plumbago? and, above all, that plumbago cannot be supposed decomposable by red precipitate, since even the nitrous acid cannot decompose it?

5thly, With regard to the power which nitrous selenite has of absorbing fixed air, I must allow the experiments of Mr. CAVENDISH to be just and agreeable to my own; but it only follows, that when fixed air is in its *nascent* state, it is more absorbable. Thus many metallic calces take it from alkalis in its *nascent* state, though in other circumstances they will take none.

Lastly, the permanence of a mixture of nitrous and common air, made over mercury, cannot be attributed to nitrous vapour, as vapour is not elastic in cold; besides, I have often made the mixture without producing any such durable vapour; and this will always happen, when the nitrous air is made from nitrous acid sufficiently diluted.

